

keepers whose carpets are in danger from the attacks of the Clothes Moth. "Take a wet sheet or other cloth, lay it upon the carpet, and then run a hot flat-iron over it, so as to convert the water into steam, which permeates the carpet beneath and destroys the life of the inchoate moth."

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Water-supply of the Metropolis

I HAVE no intention of entering into a controversy in your columns with my friend Dr. Frankland, but his letter in your impression of March 16 seems to require some reply.

When I made the remarks which are called in question by Dr. Frankland, I was careful to say that I might not unfairly be accused of having done so from interested motives, an admission of which no one who reads Dr. Frankland's letter can say that he has not taken the full advantage. I am not ashamed of my occupation, and am quite ready to admit another historical parallel afforded by Jack Cade, and confess that I, or those who have gone before me, "against the king, his crown, and dignity, have built a paper-mill." But, whether paper-manufacturers "in the exercise of what they call their rights" are polluters of streams or no, is a question into which I never entered, and is entirely beside the points which I raised.

These are in the main avoided by Dr. Frankland. The two Commissioners, a portion of whose report I criticised, and of whom it is as well to observe Dr. Frankland is one, recommend that the Thames and the Lea should be entirely abandoned as sources of supply for domestic use in London, and particularly refer to the Chalk in the neighbourhood of London, and not to the distant springs of the upper Thames as the future source of supply. In his letter to you Dr. Frankland states that "The Commissioners advise that the drinking water of London should continue to be derived from its present sources, but that it should be led away to its destination before it is mixed with the sewage of Oxford, Reading, Windsor, and other towns, and before it is fouled by the filthy discharges of paper-mills, and by other disgusting refuse." I presume that these two statements can be reconciled, but looking at the proposal that the water should be procured "within a moderate distance of London" the calculations as to the area of 849 square miles of Chalk and Upper Greensand within thirty miles of London, and looking at the enormous expense of conveying water more than thirty miles, I took that radius as representing the area out of which some district was to be placed under unnatural conditions with regard to its springs and streams, in order to supply our vast metropolis, which I am told it is contemptuous to term "overgrown." I never spoke of the fertile meadows of the Thames valley, about which Dr. Frankland makes merry, and I never intentionally alluded in the slightest degree to the main valley of the Thames, except to say that both below and above London there might be spots in it from which a limited supply of water might be pumped without much injury to the neighbouring property. My comments were intended to be confined to districts in which the proposal of the Commissioners could be carried out of sinking wells below the present spring-heads, and so constantly drawing upon them that there should be always a void below the level at which the drainage naturally escapes. If this does not mean the drying up of the streams by cutting away their natural sources of supply I shall be glad to know what it does mean.

If Dr. Frankland were as well acquainted as I am with the gravelly soil of some of the low meadows in Chalk districts, he would cease to be surprised at the possibility of their being converted into "arid wastes" by the abstraction of the water with which they are now charged up to within a very few feet of their surface. In the valley in which I live I have known the peaty soil above such gravel, even without the artificial abstraction of the moisture below, become during a dry summer sufficiently arid accidentally to catch fire and continue burning for days.

But then I am told that the wealthy City of London would be able and willing to pay for any damage it might inflict in procuring its water supply. I can only say that the word "compensation" does not occur in the Index to the Report of the Rivers Commissioners, and I have sought in vain for any allusion

to it in the text. Perhaps Dr. Frankland is not aware that at the present time the state of the law is such that even when compensation has been provided for by Act of Parliament, it has been held to be inapplicable in the case of wells being dried, on the ground that an action will not lie in respect of the loss of underground water, and therefore that no statutable damage has been inflicted.

As to the prescription for increasing the supply of spring water in a Chalk district by lowering the level of the subterranean reservoir, I may observe that in most of such districts floods are almost unknown, the soil being sufficiently absorbent to imbibe all the rain that falls, except when by chance the surface is frozen. The lowering of the water which, except in the valleys, is now usually from 100 to 200 feet below the surface, would make no difference in the receptive power of the soil on the hills, and could not be effected in the valleys without laying the streams, which now flow through them, dry.

As to London encountering the expense of a separate water supply for dietetic purposes, I can only say that if it can be effected for 2,000,000*l.*, as suggested by Dr. Frankland, it will in my opinion be far cheaper than the plan the Commissioners advocate. It is as a rule more economical to make use of what we have, than to discard all existing appliances and commence on a new system. Perhaps the Water Companies may have a word to say on this point.

The concluding paragraph of Dr. Frankland's letter seems to have been written under some misapprehension. I distinctly stated that "if we refer to the headings of Organic Carbon and Organic Nitrogen there can be little doubt of the superiority of the Kent Company's water." I may, however, be under some misconception as to the statistics under the awful heading "Previous Sewage or Animal Contamination," in which, possibly, I do not stand alone. What I ventured to suggest was that the Commissioners on the Water Supply of the Metropolis, within whose proper sphere this question lay, were not altogether wrong in reporting, that with perfect filtration and efficient measures taken for excluding from the rivers the sewage and other polluting matter, the Thames and Lea would afford water which would be perfectly wholesome, and of suitable quality for the supply of the metropolis.

If this proved impossible, then I ventured to point out that there was already in London a sufficient supply of water of the kind recommended by the other body of Rivers Commissioners.

I must not, however, waste your space and your reader's time, but will in a few words mention my principal reason for taking up this subject, which, however, apart from any such reason, I considered would be of interest to geologists.

It was this, that in an otherwise admirable and exhaustive public report, measures were advocated involving in all probability great inconvenience and loss to large tracts of country, without, so far as I could see, one single reference to such loss and inconvenience. With the advocates of a private scheme such a disregard of injury to others would be reprehensible, though possibly not uncommon, but some greater consideration of the interests involved might fairly be expected from a public document.

JOHN EVANS

Nash Mills, Hemel Hempsted, March 18

Evidences of Ancient Glaciers in Central France

MANY lovers of natural history who have not the opportunity of seeing foreign scientific periodicals, may learn the advantage of taking such a paper as NATURE in the correspondence which was published between Dr. Hooker of Kew and the late Mr. Poulett Scrope, on the evidences of ancient glaciers in Central France.

The objections raised by Mr. Poulett Scrope, and the pleasure of examining such evidences as are adduced by Dr. Hooker, have induced me to accept the invitation of friends, who also enjoy such researches, to again visit Auvergne for the purpose of examining the Mont Dore valley for glacial traces, and I would gladly avail myself of any observations made by other geologists in that region, if they would do me the favour of sending me the notes of any localities to the address below.

In the meantime M. A. von Lasaulx, of Breslau University, claims the priority over Dr. Hooker in describing glacial traces in the *Ausland* periodical, in 1872, as occurring at the entrance of the "Gorge d'Enfer." I have also before me, as I write, a travelling note-book of Sir Wm. Guise, President of the Cotswold Naturalist Field Club (date, June, 1870), in which he refers

to observing "moraines," "ice-action," "boulders," and "bloc perchés" in the same region.

My object in sending these lines to NATURE is to ask for notes of localities where glacial traces may be seen, as an aid to those who hope to examine more closely into the glacial phenomena of Central France.

W. S. SYMONDS

Pendock Rectory, Tewkesbury, March 25

Metachromism

A FEW words of explanation may seem necessary after Mr. Ackroyd's observations (NATURE, vol. xiii. p. 385) on my previous letter regarding the above subject.

The question as to whether a change of composition can be said to *produce* or to *accompany* changes of physical properties, is a matter of words which the chromium series does not affect, as the relative number of atoms of the two elements is the test of arrangement followed.

With regard to the two colour scales—one co-existent with alterations of composition, the other with alterations of temperature—I never wished to "criticise" Mr. Ackroyd's results, but solely to point out a resemblance which I had observed a few years ago, and which I was not aware that that gentleman had noticed. The two series need not necessarily be similar; and, whatever other reasons may exist for placing white in the ultra-violet, the question in hand is not whether the ultra-violet rays produce the same sensation on our eyes as a mixture of all the colours, but, Do the white compounds in question, when spectroscopically examined, *only* show the ultra-violet, leaving the rest of the range in darkness, or do they show a complete spectrum? If the first, then of course their place of classification would be in the ultra-violet; but if they give a whole spectrum (as the compounds do to which I referred), then they must be classed as having an average refrangibility greater than yellow light (because they have blue in addition to it), and less than blue light (because they have yellow also), for the centre of luminosity (on each side of which the total of light rays is balanced) falls in the green.

If we had only to deal with monochromatic substances, then of course the usual *pan-spectral* white would not need to be considered, and green (as Mr. Ackroyd says) would be the only appearance to be classed between blue and yellow.

Thus "the assertion that white comes between yellow and blue" does not "rest upon the colour relation found to obtain between the oxides of the alkali metals," though it is in accordance with the rule given on p. 347, in the six sets of the oxides and chlorides there mentioned; the sole case not agreeing with it being that of the chromium chlorides, which, however, may be accounted for.

As to the orange colour of Na_2O_2 , as Miller does not mention any colour, Turner was referred to; and if he is in error, that one instance may be laid aside; in any case it does not affect the relative natural order of blue and white.

Bromley, Kent

W. M. FLINDERS PETRIE

Socotra

WHEN I wrote the letter to the *Times* about Socotra, alluded to in NATURE, vol. xiii. p. 414, I was not acquainted with the excellent topographical memoir on this island by Lieut. J. R. Wellsted, published in the Geographical Society's Journal for 1835 (Journ. R. Geog. Soc. v., p. 129). After perusing it I am more than ever of opinion that Socotra is well worthy of the attention of the naturalist, and may probably possess many most interesting indigenous plants and animals. Unless matters are very different from what they were in 1834, there can be little difficulty in exploring the island, and if, as we are told, it has really become British property, I trust we may not have to wait much longer for some information about its zoology and botany. "Socotran Aloes" and "Dragon's Blood" are at present almost its only known natural products, and Lieut. Wellsted mentions but one native animal—a species of Civet.

P. L. SCLATER

11, Hanover Square, W., March 27

Coloured Solar Halos

SOLAR Halos such as described by Dr. Frankland (NATURE, vol. xiii. p. 404), may be seen on about seventy-five or eighty days in the year, here, and are commonest in the spring, but it is extremely rare for them to be brightly coloured. I speak

of the ordinary solar halo of about 22° radius, but the great halo of about 46° radius, is always distinctly coloured, though not a common phenomenon. It is not the "murky atmosphere" of London that hides the colours of the ordinary halo; they usually do not exist, except dull red and orange, and perhaps a faint tinge of blue. This is owing to the great breadth of the halo, which causes the colours to overlap and mix together; here it is very seldom that the halo is narrow and the colours consequently bright, as they seem to have been when seen by Dr. Schuster (p. 394). I doubt whether the name "parhelia," which he gave them, is correct; I understand that term to mean mock suns (or a bright small portion of a halo), a phenomenon visible here on thirteen days in a year on the average.

I may add that though I am rather easily dazzled, I find no difficulty in seeing halos with the naked eye.

Sunderland, March 28

T. W. BACKHOUSE

"Euclid Simplified"

MR. MORELL's defence is a curious one, and amounts to this: "If my book is a bad one I am not to be blamed, because I have copied from Amiot, Legendre, and others. If I have made blunders in derivations, &c., again I am not to blame, but to be pitied, because I could not employ better printers." As in our former notice we limited our remarks to a few only of the objectionable features in "Euclid Simplified," so, in our present notice, we shall select a few only of the points put forward in Mr. Morell's letter, though we may observe in passing, that we see no reason to retract any of our previous comments. We think that our readers will agree with us when we state our belief that Mr. Morell has utterly failed in most, if not in all cases, to appreciate the force of our objections. Mr. M. correctly quotes Dr. Wormell (pp. 78-81), but fails to see that his own statement is widely different; had he written "perpendicular to the straight line A A' through its centre" (p. 41), "perpendicular to A B through its middle point" (p. 42), we should not have found fault with him. Again, the reference to Mr. Gerard (p. 310) is not to the point; we can understand what is meant by a "segment capable of a given angle," but we still object to the term "capable angle." The revised definition of a *parallelogram* is now (see text and letter), "a quadrilateral of which the opposite sides are equal and parallel!" We did not object to the term *lozenge*, which is a well-known one, but to the way in which it was introduced.

We turned to Dr. Wormell's definition of *circumference* with some curiosity, and found that (with the exception of "plain" being printed for "plane") it was perfectly right, and that Mr. Morell had again failed to see the point in our citation of the schoolboy's definition. We contend that Amiot's sentence, as quoted by Mr. Morell, does not mean what Mr. M. makes it to mean. Dr. Wormell's use of G. C. M. is perfectly legitimate, but does not warrant, so far as we can see, the use of *R* for *right angle* (seeing it is conventionally applied to another purpose) unless, indeed, it be explicitly stated in the text that *R* is so used.

We said (p. 204) that in Theorem VI., p. 148, the reasoning is defective. Mr. Morell replies it "only errs by excess of proof." We will reproduce the "proof," and leave the decision to our readers. "The area of a trapezium A B C D is equal to the product of its height B E by the half sum of its bases A C and B D. Drop the perpendicular B E on A F, and bisect it by line G H. Produce the base A C to F, making C F = D B. Then the two triangles D H B and F H C which have for bases the base D B of the trapezium or C F = D B, and which have also the same height, $\frac{1}{2}$ B E, are equal. The area of triangle F H C = $\frac{1}{2}$ D B or F C $\times \frac{1}{2}$ B E; that of triangle D H B = $\frac{1}{2}$ D B $\times \frac{1}{2}$ B E. These triangles, having equal angles, are therefore equal. But," &c. Upon this we remark, we are not told *how* G H is drawn—the pupil is to infer that it is parallel to B D. Now we must suppose H connected with B and F, and cannot assume that B H F is a straight line, hence, though triangles F H C, B H D are equal, it does not follow that angles F H C, B H D are equal, hence too we cannot assume A B F to be a triangle. But really we must apologise for taking up space with such elementary details. For Mr. Morell's benefit we give the following:—Produce A C to F, making C F = B D, join B F, cutting C D in H, then triangles C H F, B H D are equal, and triangle A B F = A B D C, &c.

Enough has been written on this, in its present form, objectionable book. At any rate we hope that any one who has